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ABSTRACT

This speech highlights some of the problems of translating research findings, regardless of the research methodology used, into educational policy at the national level. The author discusses the problems of (1) the division of responsibility for education among Federal, State, and local governments; (2) the lack of consensus about educational objectives; (3) the policymakers' lack of faith in new research findings; and (4) the rapid pace of change in the world today. He suggests some ways for alleviating these problems. (JF)

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SOME PROBLEMS ASSOCIATED WITH NATIONWIDE EVALUATION AND THE FORMULATION OF EDUCATIONAL POLICY

by

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The preceding speakers have outlined problems associated with nation-wide cross-sectional surveys, longitudinal surveys, and experimental interventions. Each of the three types of research has the objective of guiding educational policy. Our task is to highlight some of the problems bearing directly on the complicated process of translating research findings, regardless of the research methodology used, into educational policy at the national level. The problems we shall outline are neither new to the American political scene nor unique to education, but they are perhaps more pronounced in the field of education than in other policy areas.

The first problem, perhaps too obvious to mention but too obtrusive to ignore, is the severely restricted and still unsettled boundary of federal government responsibility in the area of education. Sharing of responsibility with state and local government is not uncommon in our federal system, of course, although researchers and policy analysts from other government agencies dealing with defense or international affairs or postal services or atomic energy, etc., may tend to underestimate the problem in such areas as education. But the problem isn't simply a matter of divided responsibility. In the area of education there is little agreement about the proper lines of division. The lines dividing authority are vaguely drawn and hotly contested. There have been controversies over boundaries, and significant redrawing of boundaries, in many policy areas in the past: anti-trust, labor relations, banking, technological research and development, agriculture, to name just a few. But it seems fair to say that the controversy over boundaries of responsibility in education

has persisted over a longer time, and has generated more intense feelings, and has involved more people and more levels of government, than any of the others.

How do we translate research findings into policy in the midst of a persistent tug-of-war over policy responsibilities? There is an easy answer to this question and a more challenging answer. We believe both answers have merit.

The easy answer is to suggest that a researcher is interested in truth, not policy, and that he could care less whether educational policy is going to be made by the federal executive, or by the courts, as may be the case, or by states, or by school boards, or by administrators or teachers or students or parents, or by schools of education, or by some complex and ever-changing combination of all of these. But this answer has its limitations. First, some researchers are interested in policy as well as truth. Second, and more important, those who allocate funds for educational research are very definitely interested in policy as well as truth. And if research is to have direct implications for policy at the federal government level, then the divisions of responsibility must be taken into account. Most particularly, the reactions of other policy makers to any proposed federal policy must be taken into account. We make no apology for emphasizing this trite and obvious fact because it is often forgotten as quickly as it is admitted.

If we remembered the division of responsibility, and if we were seriously engaged in the analysis of policies, then we would recognize the need to estimate carefully the reaction of other policy makers to any

proposed federal policy. No policy analysis would be considered complete without such an estimate. No policy implications or recommendations would be given any higher degree of confidence than the confidence attached to such an estimate.

An illustration is probably unnecessary but, just in case, consider a proposed federal policy of allocating a certain amount of funds to every school district to be used for certain specified purposes in certain prescribed ways. Now it is obvious that the impact of this policy cannot be guaged without estimating how local school budgets, for example, will be modified in reaction to the proposed policy. We all know this. But this essential part of any policy analysis is often the weakest link in the chain of logic -- if, indeed, the link isn't missing altogether.

We may summarize the point by saying that policy setting in the field of education is an n-person game, where n is larger than 1. Very few policy implications are going to fall directly out of any educational research, however well conceived and executed, if the research fails to include investigation of the interactions among the n participants in the game. More systematic attention to these interactions is one of our urgent needs if we intend to have research translated into policies.

There is a second problem, perhaps still more serious and still more obvious, inhibiting the translation of research into educational policy. There is nothing close to a meaningful consensus in this country about educational objectives. We are too often mislead by the fact that we all wave the same banners and incant the same phrases extolling the blessings of education. People not only have <u>different</u> things in mind

when they stump and thump for education, though that would be problem enough; they have directly conflicting things in mind. We couldn't even agree on whether the goal of education is to develop the intellect, or to inculcate values, or to communicate knowledge, or to encourage conformity, or to raise aspirations, or to discipline, or to preoccupy, or to generate confidence, or to give joy, or to run an ever-lengthening competition for job opportunities -- and any one of these objectives is ambiguous enough to contain a host of disagreements within itself.

How are we going to translate research into policy when there is nothing but controversy over policy objectives? The educational researcher can choose among several typical responses to this problem, none of them being entirely satisfactory. An easy response, as we suggested earlier, is to decide that he is interested only in truth, not in policy. A second response is to insist that the controversy over educational goals must be resolved as the first order of business. There is a measure of logic in this response, of course, as well as a large measure of futility. Even the youngest of us here today won't be a witness to the resolution of this conflict. Incidentally, we don't cite this difficulty of achieving consensus as an indication of the cantankerous quality of the human species. There is no reason why people should ever agree on educational goals.

A third response to the problem is to insist that some authority should list goals and priorities, arbitrarily if need be, for guiding research and policy analysis. This may sound like a reasonable, pragmatic solution to the analyst's needs, but it has its limitations.

We all know that a listing of vague and ambiguous goals is not too helpful. But a listing of very precise goals is not always helpful either.

Even if different goals in the list are not directly conflicting, they are more than likely to be competitive. Certainly the pursuit of any one goal will use time and attention and resources that might have been devoted to the pursuit of other goals. If an analyst doesn't know this simple fact of life at the outset of his research he will discover it soon enough, and then he may think the problem can be solved by assigning priorities. He is still deluded, though this is a common enough delusion.

We haven't really answered very many practical issues by deciding to list one educational objective first, another second, another third, and so on. We aren't going to direct all of our efforts toward the attainment of the first priority objective, nor even the top two or three objectives. We are going to compromise them all and distribute our efforts over a broad range. The challenge facing policy makers is to decide how to distribute the effort, and the challenge of policy-relevant research is to help guide that decision.

We can summarize this second point we are making by saying that, if research is to have direct implications for educational policy, then researchers should neither attempt to evade the question of educational objectives nor should they adopt simple answers to the question. Instead, more effort should be expended to satisfy these two needs: first, the need to find out what different educational objectives people have in mind, and how strongly they support these objectives, and why; and second, the need to find out how much of any one objective must be sacrificed in order to pursue any other objective. These are researchable questions, requiring data, not arbitrary value judgments, and they are questions for which policy-makers need answers.

A third problem associated with nation-wide evaluation and the formulation of educational policy is the problem of confidence. Given the current state-of-the-arts in educational research, and given our current limited knowledge about educational effects, there is a certain reluctance on the part of many policy makers to place implicit faith in new research findings. Even when we learn something important -- or when we think we have learned something important -- we are frequently met with widespread skepticism.

It is time that policy makers dismiss most quickly those findings which they don't like. And it is true that policy makers will accept many other findings which appeal to them regardless of the strength of the evidence. Policy makers are like that. Parents and teachers and students and administrators are like that, and perhaps educational researchers too. Ambiguity of findings is always taken by some people as an excuse to believe what they want to believe.

This problem is not at all unique to the educational policy arena. It exists in the arenas of public welfare, international relations, ecology, health, and any other you may name. Educational researchers can alleviate the problem by becoming familiar with the common rationalizations of policy makers and anticipating their diversionary reactions to important research findings and marshalling evidence to head off such diversions.

Since the educational system is vastly complex and since no two students or teachers or classrooms are alike, the number of things we know with absolute certainty about education is rather limited. Policies must, inevitably, be guided by rough estimates as well as by precise measures,

and by reasonable hypotheses as well as by confirmed theories. But once we leave the bedrock of well-tested theories and start building our educational system on less solid ground, we sometimes seem to lose touch with the ground altogether.

Perhaps a practice of our educational system itself is at fault here. We try to teach students to distinguish what they know from what they don't know, and while that sounds like a good idea indeed, perhaps we unwittingly neglect important matters of degree. If we tried to divide up the realm of educational policy questions into two categories, those questions we really know the answers to and those we know nothing about at all, there wouldn't be many questions in either category. There is, after all, a middle ground between what we fully comprehend and what completely mystifies us. There are educational hypotheses for which there is persuasive, but not compelling, evidence. For the next half century at least, educational policies are going to be made on the basis of what we <u>think</u>, not what we <u>know</u>; that is, on the basis of judgments. Obviously we shall be more confident of some of our judgments than we are of others and that forces us to deal with the matter of degrees of confidence.

We are not concerned here simply with computing standard errors of estimate or confidence intervals or the like, although that is a gesture in the right direction. We are concerned with far more difficult questions. For example, how much confidence should a policy maker have that surveys of first graders in 1972 have valid implications for first graders in 1982? How much confidence should a policy maker have that findings of an experiment under certain controlled conditions will apply in somewhat altered conditions? How much confidence should he have that observed short-run efforts of an

educational innovation won't be overbalanced by unobserved long-run effects. There is absolutely no way to translate research into policy without making a host of difficult judgments such as these. And that is where the controversies, the uncertainties, the confusions, and the disappointments come into the formulation of educational policy.

We may summarize this third point by noting that educational policy makers need more than raw research data as grist for the policy-making mill. They need confidence appraisals of research findings, and confidence appraisals of the many possible interpretations and policy implications that are drawn from the findings. Supplying these appraisals is a challenging task deserving as much effort, and ingenuity, and care, and quality control, and criticism as the task of conducting basic research itself. We believe that the challenge receives only haphazard attention and that the research community as a whole should undertake a major responsibility for bringing more systematic analysis to the problem.

We shall mention but one more problem concerning nation-wide evaluation and the formulation of educational policy, and that has to do with the fact that the world is changing. We doubt that any of you will challenge this assertion.

Change at a slow pace is only a problem for the disadvantaged, but change at a rapid pace can be a problem for everyone, including the policy makers and researchers. Policy makers can adapt to slow changes by waiting for problems to manifest themselves; by making incremental changes in laws and budgets and programs; and by testing these incremental changes in actual use, revising them as necessary. Researchers can adjust to slow changes by making incremental revisions in their assumptions and by

gradually shifting the focus of their attention. Rapid change is likely to overwhelm these incremental adaptive processes and leave both policy makers and researchers hopelessly outdated.

We don't intend to dwell on the many obvious problems confronting the researcher as he tries to keep up with the changing world. Instead, we want to point out that the challenge of translating research into policy takes on a very different character once we begin to modify our customary incrementalist policy-making style in response to the pressures generated by rapid change. We will mention two modifications in policy-making style and note the special problems they will generate for the research community.

The first modification in policy-making style is in the direction of longer lead times and more comprehensive policies. Faced with increasingly rapid change the policy makers discover that they need to focus on the future, not the present, and they discover that the solution to developing problems often require integrated and complex packages of changes rather than a few isolated incremental changes. In other words, the policy makers will start searching for alternative blueprints for the future as opposed to minor modifications of the present.

We may have the best education research in the world, but we aren't going to find it easy to supply the blueprints that the policy makers want. It is difficult enough to alter some single variable in the current educational scene and to observe and measure its effects. If the policy makers want to know what will happen if a dozen important variables are altered simultaneously, who is going to tell them? Yet, they need to know! The capability of our society to cope with the increasingly rapid rate of change may hinge upon

the capacity of our researchers to predict the consequences of numerous simultaneous changes.

If the challenge of designing and orchestrating comprehensive changes seems too great, policy making styles can be modified in a different direction. We can open up opportunities for widespread innovation and variation, and we can attempt to manipulate the incentive structure so as to encourage promising developments. This is the policy style that has been most prominent in this country in the economic sphere. This policy style imposes different demands on the educational research community. The need here is for assessment of the impact of truly decentralized decision making, and of wide variations in educational practice, and of continuous change, and of coordinated modifications of the complex of incentives operating on the system. It is a different style of educational research than is required for the development of alternative blueprints, and either of these styles is different from that characteristic of most of today's educational research.